Technical Comments

Comments on "Satellite Potential in an Ionized Atmosphere"

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In the April 1965 issue, the AIAA Journal published an article by N. C. Jen[1] on the problem of a spherical satellite moving in an ionized atmosphere. Since full-length journal papers are subject to "rigorous technical review," I was distressed to find in this paper a number of serious errors which apparently escaped the attentions of the editors.

Jen considered a collisionless plasma flow about a sphere. His goals were to obtain the self-consistent potential field distribution and in particular the satellite potential, the "Coulomb drag," and the wake trail patterns. In this note, I shall point out only the major errors.

1) Jen assumed that the potential dropped rapidly from the value at the satellite wall to a negligible value within a thin sheath of the order of several Debye lengths. External to the sheath, the region was considered field-free, and the charged particles behaved as neutrals. Since the sheath was assumed thin, the edge of the sheath then forms a near sphere of a slightly larger radius. By a somewhat tortuous but completely false argument, Jen arrived at the conclusion that the sheath acts as a specularly reflecting surface (item 3). At this point, one can already conclude, within Jen's model so far, that the solution of this problem in the allegedly field-free region is identical to that of a collisionless neutral flow over this near sphere of specularly reflecting surface. In particular, the drag will be completely determinate and independent of the satellite potential. However, the number density distributions of ions and electrons so calculated in the allegedly field-free region will be different since the speed ratios of the two flows are different. Thus, we arrive at a contradiction to his field-free assumption via Poisson's equation.

2) Jen assumed that the charged particles that struck the satellite wall would first recombine, then would be "dissassociated," and be "diffused out or reflected back (as charged particles), from the satellite surface...." This novel model has never been suggested in the literature and is, therefore, an original alternative to replace the usual assumption that the charged particles simply recombine and are reemitted as neutrals. Note that Jen's new model gives rise to no net current regardless of the satellite wall potential, and his later requirement of zero current to the satellite in Eq. (13) becomes difficult to explain. Furthermore, Jen's statement that "the striking velocity and the reflecting velocity (of the charged particles) must be the same from the statistical view point," is, of course, completely false.

3) In requiring zero current across an elementary area on the satellite surface, Jen wrote Eq. (13) to determine the satellite wall potential (without justification or derivation):

$$(C_+ + W\cos\theta)e^{q\psi_0/kT} = (C_- + W\cos\theta)e^{-q\psi_0/kT}$$

where C_- and C_+ are mean thermal speeds of electrons and ions, W is the speed of the satellite, and ψ_0 is the satellite wall potential. Ignoring the difficulty mentioned in 2), we see that Jen has a satellite made of a nonconductor since ψ later turned out to be a function of θ. Furthermore, when $C_- > W > C_+$, at the rear of the satellite where $\cos\theta < 0$, no solution for ψ_0 exists. For precisely this case, however, Jen states that "ψ_0 becomes extremely large" and on the strength of this observation, rests all his conclusions about the wake trail befavior.

4) To compute the so-called "Coulomb drag," Jen invoked the following relation:

$$p = n_i kT$$

where p is called pressure intensity, n_i is the ion number density, and T is the undisturbed ion temperature. His Coulomb drag was obtained by integrating the component of this pressure force acting on the satellite wall. For good measure, Jen also suggested that, depending on whether $W \gtrless C_+$, the rear part of the satellite may or may not contribute to this integration. His Coulomb drag depends exponentially on ψ_0.

The previous criticism is by no means complete. For example, Jen somehow saw fit to invoke Boltzmann's H Theorem to justify Eqs. (6) and (7). No useful purpose will be served to identify all of the controversial points. It is difficult to believe that all of the points raised here could have been overlooked if this paper has, indeed, had a rigorous technical review.

Reference

[1] Jen, N. C., "Satellite potential in an ionized atmosphere," AIAA J. **3**, 714–717 (1965).

Received July 26, 1965.

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Reply by Author to S. H. Lam

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Lam has apparently misunderstood the whole paper but made a "vigorous technical review" of "collisionless neutral flow about a sphere" which is significantly different from the paper. The paper clearly indicated that the particles are electrically charged. In the field-free region, these charged particles are considered to be Maxwellian with large mean free paths. The plasma has the basic property of electrical neutrality.

The mechanism of reflection of electrons from the sheath has been analyzed and agrees with Spitzer's statement[1]: "In equilibrium a potential gradient arises near the wall, reflecting most of the electrons into the plasma, the number striking the wall being equal to the corresponding number of positive ions reaching the wall." This paper also indicated that the equipotential surface is nonspherical. On the other hand, Lam made a number of statements, such as "one can already conclude within Jen's model so far that the solution of this problem in the allegedly field-free region is identical to that of a collisionless neutral flow over this near sphere of specularly reflecting surface;" "the edge of the sheath then forms a near sphere of a slightly larger radius;" and "we thus arrive at a contradiction to his field-free assumption via Poisson's

Received September 13, 1965.

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equation." The author really wonders if Lam has read this paper thoroughly.

This paper also has indicated no intension to investigate the complicated processes between the striking particles and the satellite surface, but employs only the conditions of steady state of satellite potential and constancy of satellite mass. The diameter of the satellite has been considered to be rather large, of the order of meters. In general, such a satellite is constructed as a thin conducting shell. It is obvious that the electrical current normal to the satellite surface must be zero. Spitzer[1] also stated that "if the potential of the solid surface is allowed to float, no current must flow from the plasma to the surface." In Eq. (13), $n_{oe}e^{+g\psi_0/kT}$ and $(C_- + W\cos\theta)$ are the electron number density and their total mean speed in the direction normal to the satellite surface. The product of these two quantities is the contribution to electric current by electrons. In the same way, the left side of the equation is the contribution by ions. For zero current, Eq. (13) is established easily.

Lam's statement of "we see that Jen has a satellite made of a nonconductor" is certainly not true, since the paper has not used the term "nonconductor" at all, either explicitly or implicitly. Lastly, Lam's item 4 on Coulomb drag even contradicts his own item 1.

Reference

[1] Spitzer, L., Jr., *Physics of Fully Ionized Gases* (Interscience Publishers, Inc., New York, 1956), p. 17.

Comment on "General Instability and Optimum Design of Grid-Stiffened Spherical Domes"

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Grid-stiffened spherical domes subject to external pressure, as suggested by Crawford and Schwartz,[1] have been designed and built for a number of years. Shells with base diameters of over 100 ft have operated successfully as roofs, parts of space simulators, and outer walls of cryogenic vessels.

Theoretical results and experimental data for general instability and for buckling between stiffeners have been published by the author.[2–4] The theoretical results[3] for a stiffened shell give the critical buckling pressure for general instability as

$$p_{cr} = 0.365E\,(t_s/R)^2\,[1 + (12I/t_s^3b_s)]^{1/2}\,[1 + (A/t_sb_s)]^{1/2}$$

when I is the effective moment of inertia of the stiffener, and A is the area of the stiffener.

The critical buckling pressure for local instability[3] is, approximately,

$$p_{cr} = 7.42Et_s^3/Rb_s^2$$

when the torsional stiffness of the stiffeners is relatively high. When the torsional stiffness is relatively low, the local buckling pressure can be calculated by using the method previously published.[4]

The results of tests[2] indicate that if local buckling occurs at a relatively low pressure, general instability soon follows, and the theoretical general stability pressure is very low. In addition, edge effects are important in the design of stiffened shells. A series of tests conducted at the University of Missouri indicate that general instability will occur at a very low load if poor edge conditions are present. In order to reach the theoretical value of the buckling pressure, the yield strain of the material must be high enough. Even though the strain in the shell and stiffeners just prior to buckling might be relatively low (considerably below the yield strain of the material), the strains during and after buckling are relatively high (exceeding the yield strain) in a practical shell.[5] This pseudoelastic effect has been demonstrated in tests performed at the University of Missouri.

The connections between the stiffeners and between the stiffeners and the shell are of considerable interest to the engineer. A limited number of tests have shown that the general instability critical pressure is only reduced about 10% if the connections between the stiffeners are eliminated and if the attachments between the stiffeners and shell are only 50% effective.

Received May 26, 1965.

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References

[1] Crawford, R. F. and Schwartz, D. B., "General instability and optimum design of grid-stiffened spherical domes," AIAA J. **3**, 511–515 (1965).

[2] Buchert, K. P., "Stability of doubly curved stiffened shells," Ph.D. Dissertation, University of Missouri, Columbia, Mo. (January 1964).

[3] Buchert, K. P., "Stiffened thin shell domes," AISC, **7**, 78–82 (1964).

[4] Buchert, K. P., "Zur Stabilität grosser, doppelt gekrümmter und versteifter Schalen," Stahlbau **2**, 55–62 (1965).

[5] Buchert, K. P., "Buckling of doubly curved orthotropic shells," Engineering Experiment Station, University of Missouri, Columbia, Mo. (November 1965).

Reply by Authors to K. P. Buchert

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Stiffened shells of various types have been designed, built, and used in flight vehicles as well as in civil structures for a number of years; however, there still exists a potential for further reducing their weight and improving the accuracy of their analysis. Substantial improvements in vehicle performance that can be gained from such weight reductions provide the impetus for accurately defining the potential minimum weight and associated design details for the many classes of stiffened shells used in their construction.

The general instability formula presented in the previous comment is an approximation to the critical pressure for symmetric buckling with a reduction factor derived from a Karman- and Tsien-type buckling analysis. Perhaps Professor Buchert's procedure is justifiable for monocoque or sandwich shells whose symmetric and asymmetric modes of buckling have equal critical pressures; however, the subject paper shows [Eqs. (13) and (14)] that the critical pressure for asymmetric buckling is lower by a factor of $[(1 + D_3/D)/(1 + E/G_3)]^{1/2}$ for equal stiffeners in the orthogonal directions when $D_3/D < E/G_3$, as it is for the square-grid-stiffening case. In terms of parameters similar to those used by Professor Buchert previously, the small-deflection theory formula

Received September 23, 1965.

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